In making the tests, an *A* flower cluster that was fragrant to me was used in contrast with a *B* flower cluster that was adjudged fragrant by Mr. Avery or by one who had been found to react to it in the same manner in which he did. The person to be tested was asked to decide which of the two was the more fragrant. There was an amusing uniformity in the manner of response. The subject would generally say he feared he was not smelling well that day, would then blow his nose and almost at once pick out either *A* or *B* and wonder how any one could think the other fragrant. When questioned as to fragrance in the flowers that were not preferred, he would generally say they were not fragrant but had a slight odor variously described as being a plant odor or an odor like a dead leaf.

The pleasure obtained from odors is often closely bound up with other associated perceptions. For this reason, in some cases the individuals tested were asked to smell the flowers with their eyes closed. Color associations were shown to have no controlling influence in the reaction. In some instances the tests were repeated but without affecting the results.

Of the men, 17 preferred the flowers of *A* while 9 preferred those of *B*—a ratio of 2 to 1. Of the women, 9 preferred *A* while 4 preferred *B*. In general the results were clear-cut and the individuals tested found fragrance in one of the two flowers and not in the other. A few, however, found a slight fragrance in the flowers that they did not prefer and two women found fragrance in both and could not decide between them.

Flowers from the two plants were exhibited at a staff meeting of the Carnegie Station and were repeatedly smelled by the seven members present. Five found fragrance in A and not in *B* and two showed a reversed reaction.

It is a trite proverb that in matters of taste there can be no argument. The assumption is that though we differ in our preferences, our perceptions are essentially the same. In the case of the verbena flowers under discussion, however, it has been shown that preferences of different individuals in regard to fragrance are based upon radical differences in their perception of odors. The condition suggests color-blindness, but those who are color-blind react to both of two colors when they are unable to distinguish between them. About two thirds of the individuals tested with the verbena flowers were "blind" to odors in the flowers of plant *B* while perceiving odors in *A*. On the other hand, about one third were "blind" to odors in *A* while perceiving odors in *B*. It is as if my black looked white and my white, black to Mr. Avery and his group; while from his viewpoint, I and the group that agreed with me were equally distorted in our vision.

It is well known that people differ considerably in their ability to hear tones of higher musical pitch. Many can not hear the notes of the cricket. Other insects produce sound vibrations of so high a pitch that they are inaudible to any human ear, though perceived by related insects. The peculiarity in the perception of the verbena fragrance might resemble the individual peculiarities in the powers of hearing if it were true that a large group of people could hear the extremely high musical notes and not the lowest tones while another group could hear the lowest and not the highest.

The acts brought out in the foregoing discussion furnish an added example of the difficulty in classifying characters studied in inheritance. A group of different individuals in investigating fragrance in our pedigree of verbenas would be classifying their own olfactory perceptions as well as the actual odors in the flowers. It is well for us to recognize the limitations of the personal equation. Discrepancies in conclusions reached by different investigators may not be due to any fault in logical reasoning or to lack of intellectual honesty. Their diverse conclusions may be inevitable, given only differences in their sensory reactions and in their mental experience.

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THE WHITE-SPOT DISEASE OF ALFALFA

FOR a number of years the writer has observed the white-spot disease of alfalfa, par-

ticularly in the middle-western, intermountain and Pacific Coast states. Usually this disease is not considered serious by alfalfa growers, but in many instances the writer has noted that the disease may be more or less disastrous and may produce a very decided loss in yield. During the last few years particular attention has been paid to this disease because of its very great prevalence in the intermountain states. In the Salt Lake Valley, Utah, this disease has been considered by many of the farmers as being due entirely to the smoke from the smelters. However, the writer has found it to be quite as serious in districts far removed from the Salt Lake Valley where soil and climatic conditions are the same. Because of the importance of the disease the writer has made some studies of which a preliminary report is given below.

Reference to the literature indicates that very little has been done to determine the real cause of the disease. The earliest reference to the disease is that by Stewart, French and Wilson.[1] These authors indicate that they believe this disease to be due to a physiological disorder of some kind. The next references are by Reed & Crabill[2] and by Clinton.[3] The most recent reference is by Crabill,[4] who believes that white spot is due to the wounding of the tissue of the crowns of the plants. His experiments indicate that by cutting away a portion of the tissues the typical white spot was produced. The occurrence in nature, he believes, is due to the fact that the injury to the plants is produced in the late fall or winter because of the fact that he has only observed the disease in the early spring. The wounding of the plants in cultivation, he believes permits the entrance of certain fungi which tend to rot the crowns and later the roots. Such plants, he has found, will show white spot in the early spring, shortly after the plants have started to grow. Recently, however, Crabill indicates (letter of April 17, 1918) that he did not always find a crown rot of the affected plants and he thinks that crown rot may, therefore, be only a circumstance and, after all, not the true cause.

In carrying out some experiments on the treatment of soils with various water-soluble substances, the writer, to his surprise, found that white spot suddenly appeared in a large number of the treated plots. Each plot covers an area of 25 square feet, composed of fifty plants from two to three years old. The plants have been very carefully cultivated with a hoe and the crowns have never been injured in any way. In general the white spot appeared within sixty to seventy hours after the soil had been treated. In no case did a single specimen of white spot appear in the check plots. Furthermore, white spot did not appear in any of the plots where the total water-soluble substance applied was below a certain amount. Further experiments indicated that the "soil solution" alone would not produce white spot, but that the factors of soil temperature, atmospheric temperature, relative humidity of the atmosphere and light are important. In other words, it requires a certain coincidence of these various factors at what we shall term the optimum before an effect was produced upon the plants such as would cause white spot to appear.

It may be stated here that the experimental plants are growing in a sandy-loam soil and at no time previous to the experiment had white spot appeared.

The work has progressed to the point where the writer believes that the osmotic pressure of the soil solution is one of the important factors in the production of white spot, not only under experimental conditions but under field conditions as well. With conditions for transpiration at the optimum, lessening or preventing endosmose, by reason of a soil solution having a higher osmotic effect upon the cells of the transpiring organs. The degree of injury produced will depend upon the factors enumerated above, together with the time factor which is all important. If these factors are

[1] "Troubles of Alfalfa in New York," by F. C. Stewart, G. T. French and J. K. Wilson, Bulletin No. 305, November, 1908, New York Agricultural Experiment Station, Geneva, N. Y.

[2] Va. Station Technical Bulletin, 2, 39, 1915.

[3] Conn. Sta. Report, Report of the Station Botanist, 1915, 425.

[4] *Phytopathology*, Vol. 6, No. 1, 1916, pp. 91.

coincident for a relatively short period of time only, a few white "spottings" at a distal end of the leaflets may result; if the time element is lengthened, all the cells of the leaflet may suffer a water loss much below the wilting coefficient, and instead of a "spotted" appearance the entire leaflet will bleach white. This is exactly what happens under both experimental and field conditions. In the intermountain country where a very large number of observations have been made, it has been noted that fields showing a considerable incrustation of alkali when irrigated exhibited white spot in more or less amount, depending upon the other environmental factors above mentioned. Also, a sudden rise of the water table in irrigated districts has brought about the same appearance of the plants in the fields. Some very interesting observations have been made on fields adjacent to each other, with plants of the same age and all conditions the same excepting the application of water. The irrigated fields showed extensive white-spot trouble, while the non-irrigated fields showed none.

It has been noted by eastern pathologists who have made observations on this disease that it occurs mainly in the spring of the year. However, the writer has observed it in the intermountain districts during the early spring, during mid-summer and during the late fall; in short, throughout the entire growing season.

Specimens of artificially produced white spot of alfalfa were submitted to several plant pathologists who reported that these specimens were identical with diseased alfalfa plants which they had themselves collected.

An extended report will be published in due time after the completion of certain experiments which are now in progress.

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THE POLYHEDRAL VIRUS OF INSECTS WITH A THEORETICAL CONSIDERATION OF FILTERABLE VIRUSES GENERALLY

IN a previous paper[1] J. W. Chapman and I called attention to the fact that the wilt or polyhedral disease affects many different species of insects. We also showed that the disease is not produced by bacteria, but is caused by minute organisms capable of passing through diatomaceous filters, and further attempted to demonstrate that the polyhedral bodies always found associated with the disease are not organisms, as supposed by Bolle, Fischer, Marzocchi and Knoche, but reaction bodies—simply nucleoprotein by-products.

In order to satisfy myself that we were really dealing with an organism and not merely with an enzyme, toxin or other material a large series of passage infections were instituted. Twenty-five gipsy moth caterpillars were infected at a dilution of 1:1,000 with material obtained from a caterpillar previously dead of wilt. The animal was merely ground up, sterile water added and the whole filtered through a sterile Berkefeld grade "N" candle. All twenty-five caterpillars fed with the filtrate died typically of wilt during the course of three weeks, whereas twenty-five controls fed with the autoclaved filtrate lived, pupated and transformed into moths. One animal dead in this first series was prepared, the material diluted as before (1:1,000), filtered and fed to another series of twenty-five caterpillars. The experimental animals all succumbed, whereas the controls did not. Third and fourth passage infections were performed and the results were similar with the exception that the period from infection to death was considerably shorter at the fourth passage than at the first three. This shortening of the time between infection and death seems to point towards an increase in virulence with successive passages.

There are certain autocatalytic substances like chromatin that increase progressively, so to the physiologists my passage infections may not necessarily be proof for the contention that I am dealing with parasitic ultra-microscopic organisms. However, if one reviews the field of the filterable viruses[2] and compares all of the results obtained by other workers with my results, one can not but feel inclined to

[1] *Biol. Bull.*, Vol. XXX., No. 5, pp. 367–390.

[2] Thirty-two diseases are now known to be caused by filterable viruses.